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Nicolas Bouleau

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MATHEMATIZATION OF RISKS
and economic studies in global change modelling

Nicolas BOULEAU*

Abstract

With respect to the climate change, and more generally to the energy problem, in the laboratories working on the subject, scientists contribute to clarify the situation and to help decision makers by yielding and updating factual physical informations, and also by modelling. This conceptual work is mainly done in the language of economics. This discipline, which appears therefore in the core of the reflecting process in action at present, is however rather peculiar is the sense that it uses mathematics in order to think social phenomena.

It is on this methodological configuration that we hold a philosophical enquiry. Our analysis focuses on risks, incertitude and on the role of mathematics to represent them. It concludes on the importance of a certain type of modelization, investigation modelling, which reveals new significations.

This study attempts to enlighten some part of the limit between mathematized knowledge, as economics, and interpretative meaning used in every day life and social and human sciences

Keywords : risk, tails of probability distribution, finance, interpretation, VaR, modelling

I. The question of the tails of probability distributions.

The probability distributions are badly known in the region where the probability is low, in particular in the neighborhood of infinity. Although well known today, the awareness of this important question has been progressive during the history of sciences and explains some present difficulties.

First of all, it has been remarked that asymptotically thick distributions possess strange unusual properties. The discovery of the fact that the law $\frac{dx}{\pi(x^2+1)}$ (today called Cauchy law) has no expectation and has the stable property, hence doesn't fulfilled the central limit theorem, goes back to Siméon Denis Poisson around 1830, twenty years before Cauchy. Henri Poincaré in his probability course at the Sorbonne at the close end of

*CIRED, Ecole des Ponts ParisTech, `bouleau@enpc.fr`

the nineteenth century notes that this type of distribution contradicts the least squares method. Nevertheless during the twentieth century, with the development of mathematical economics, operational research and decision theory, the quadratic mean reasonings expand in a whole general rationality.

It may be sketched in the following manner: if, for a decision in uncertainty, you choose the value of a parameter so that the mean of the squares of the discrepancies between this quantity and the chosen value be minimal, then this kind of choice is compatible with other subsequent choices taken with the same method and with possible sub-choices done along the same principle. It is what mechanicists know when a solid body is shared into several parts, the gravity center of the gravity centers of the parts weighted with the masses of the parts, is still the gravity center of the whole body. This rationality impregnate economical reasonings of optimal decision as soon as they involve the notion of expectation or of conditional expectation which carry in themselves the idea of mean quadratic minimization.

A remark must be done here that the use of utility functions in order to escape from the concept of mathematical expectation, although yielding true services to represent the behaviour of the agents with their subjective probabilities (and avoid St Petersburg paradox), doesn't solve at all the problem of the tails of probability distributions because the utility function also is badly known on extreme events.

On one hand the reasonings in quadratic mean are iteratively coherent and develop in a whole rationality, on the other hand, procedures and arguments using thick probability tails build a language which tends to deteriorate, what leads necessarily to methodological and philosophical questions: given a family of probability distributions P_α on $\mathbb{R}$, each P_α possessing an expectation, if the real parameter α is random with law $\nu(d\alpha)$, it is possible that the resulting probability distribution

$$P = \int P_\alpha[\,.\,]\ \nu(d\alpha)$$

have no expectation. This is not related to the existence of an expectation for α, but comes only from the fact that an everywhere finite function may be non integrable. This has been known for long time and concrete decisional consequences have been emphasized by Mandelbrot in the 1970's (Mandelbrot 1970 and 1973) and more recently by Weitzman (2007) in climate change models about variances what is similar. In other words, the intersubjective rationality based on the least squares is fragile, when you leave it a little, no return force leads you back to it.

To these remarks, we must add specific features related to the identification of models by mean of statistical experiments[1]. Of course no histogram is able to yield the tails of the distributions and this happens also, in the multivariate case, for estimating correlation in any form of this notion (copulas, etc.). When the dimension is high, as usual in physical or economical models for global change, to the preceding difficulties comes in addition the

[1]Cf. K. Burdzy 2009 for different philosophies of probability theory and the question of the use of statistical estimate for predictions.

famous phenomenon of the “curse of dimensionality” which makes identification much more fuzzy[2].

Let us mention also that the use of probability distributions on $\mathbb{R}$ or $\mathbb{R}_+$ to evaluate probabilities of exceeding a threshold thanks to the extreme value theory is most often a doubtful operation because the extreme value theorem yields a convergence in distribution to a limit law which depends on the scale of the measurement device. Except for very particular physical cases where the quantities to be measured are clearly additive, the obtained thresholds have no intrinsic value and depends on the choice of a linear or logarithmic scale for instance[3] .

In order to tackle the methodological difficulties related to the tails of probability distributions, we will enlarge the problem and open new angles of attack.

II. Teachings drawn from the mathematical finance.

Finance developed since the 1970’s several techniques of risk management, which are interesting examples both to understand their efficiency and to attempt to draw lessons of their weakness as recently revealed by the subprime crisis. The amounts staked on the markets are finite but not a priori bounded, and the problems of tails of distributions occur for all models, either models of options pricing and hedging, for interest rate models, or for assessing the exposure to risk of a financial company.

Slightly after the development of the organized derivative markets (the 1970’s in the US, the 1980’s in Europe and Japan), under the impulse of the bank JP Morgan, appeared a simple principle in order to evaluate the exposure of a financial organism: the “value at risk” denoted VaR represents the maximal loss that is not exceeded with the probability p (in general $p = 95\%$ or $p = 99\%$) at a given date. The computation of VaR for an assets portfolio or for a bank presents several difficulties due especially to the restlessness of the spot prices (mark to market) and the discounting renormalization of term products. Practically, at present, in the banks and in most of the textbooks, VaR is computed with the help of explicit probability distributions calibrated on reality by usual statistical parametrical tests, hence hypotheses on tails of distributions are assumed. However, in its philosophy, the criterion VaR doesn’t involve at all the tail of the distribution and its logic may be extended further by the following principle: to have a good knowledge of the parameters when they are in some domain and to content oneself with an upperbound of the probability of the exterior of the domain. We shall come back to this idea later on.

At present, the use of VaR is going to be supplanted by more sophisticated techniques.

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[2]Actually, from a pragmatic point of view, in concrete cases where the tail is unknown, even the total mass of the distribution may be indeterminate. Our measurement devices are not always designed to distinguish accurately between very large and infinite quantities. In other words, it may happen that we hesitate about the possible presence of a mass at infinity. This might be written as usual in the theory of Markov processes when dealing with sub-Markov kernels.

[3]For other pecularities of this theorem see N. Bouleau “Splendeurs et misères des lois de valeurs extrêmes.” *RISQUES* 3, p 85-92 (1991).

These new methods appeared to avoid a default of the criterion VaR. The criterion VaR is not sudadditive in general. If $VaR(X_1)$ and $VaR(X_2)$ are the absolute values of the maximal losses on the portfolios X_1 and X_2 at some given date and with some given probability threshold, for the union of the two portfolios, where compensations may occur, we would expect to have $VaR(X_1 + X_2) \leq Var(X_1) + Var(X_2)$ but this inequality is not fulfilled in general[4].

In order to correct this default "coherent measures of risk" were proposed[5]. It is possible to show that any risk criterion satisfying the common sense rules that are expected when two portfolios are gathered or when a portfolios is multiplied by a deterministic constant, (subadditivity $C(X_1 + X_2) \leq C(X_1) + C(X_2)$; homogeneity $C(\lambda X_1) = \lambda C(X_1)$; monotony $C(X_1) \leq C(X_2)$ if $X_1 \leq X_2$; invariance by translation) are necessarily of the form

$$m(L) = \sup\{\mathbb{E}_P[L] \mid P \in \mathbf{P}\} \tag{1}$$

where $\mathbf{P}$ is a set of probability laws, $\mathbb{E}_P$ the expectation symbol under the probability P and L the loss (i.e. the opposite of the value of the portfolio $L = -X$).

An interest of this result is to emphasize the importance of scenarios. The probability distributions which govern financial assets generally depend on the observer. The coherent measures of risk suggest to consider several probabilistic hypotheses to perform the risk analysis and to take eventually the most unfavorable estimate.

The mean loss above a threshold (expected shortfall), and all the coherent measures of risk, because of the formula (1), need the knowledge of the whole probability distribution of the loss, including the tail with its very rare events. They yield therefore numerical quantification criteria satisfying more harmonious logical principles but carrying the congenital default of throwing mathematical writings on a partially or totally unknown reality.

This leads us to deepen the reflection on the over-mathematization of the finance today, because this is the source of more general teachings. The recent crisis is the convergence of several causes which are not our purpose to examine exhaustively. But it reveals a fundamental point about risks and their mathematization. The above mentionned techniques of risk quantization favored the organization of markets of credit risks, that denotes the term of *titrization.* When insurance companies or banks have to manage risks in their country or region and if these risks are independent from a region to another or less correlated, they have advantage to exchange their risks. In order to install a market of risks it is necessary that sellers and buyers be able to quantify their own risks and they use for this the mathematization that we described above. The idea of organizing re-insurance as a market whose participant are insurance companies and where the product to be exchanged is the risk is an old "genious" idea studied by Maurice Allais and Kenneth Arrow in the 1950's. It applies as well to banks for the management of all their risks[6]. In Arrow's model, each participant has his own subjective probabilities. This extend to the

[4]Cf. e.g. R. Portait and P. Poncet, *Finance de marché*, Dalloz 2008, p. 901 *et seq.*

[5]Cf. Artzner, et al. 1997 and 1999; Delbaen 2000.

[6]Cf. K. Borch "Equilibrium in a Reinsurance Market" *Econometrica* vol 30, n3, July 1962; and R. Portait and P. Poncet *op. cit.* p. 931 *et seq.*

case where everyone looks at several scenarios to evaluate his risk. Since agents do not have the same measure of risk because they dont interpret the underlying reality to each asset in the same way and because they have different risk aversion and utility functions hence different behaviors in uncertainty, exchanges of risks against payment may be profitable for buyers and sellers. Prices may be computed thanks to mathematical models.

On such bases, credit risk markets have developed especially in the US these last twenty years.

For the management of completely probabilized risks (known probability distribution and known function of damages) the interest of risk markets are clear. It amounts not to put all one's eggs in one basket. This would be the case for instance for the material damages of traffic car crashes. But most of risks possess a purely semantic and interpretative component which is based on *a reading* of economy and which remains in every case concerning the tails of distributions. For such risks, to place one's confidence in the common opinion – as done by putting the risks in market – is a logical mistake, since most of participants go by a ready-made mathematization drawn from notation agencies, and therefore do not bring anything to the lack of understanting of anyone. In other words, putting the risks in market belongs to a rationality only for completely calibrated and perfectly statistically known situations.

The probabilistic representation of risk (with objective probabilities or subjective probabilities from the points of view of the agents) is classically a pair of mathematical quantities, 1^o a probability distribution which governs the state of nature that may happen, 2^o a function which to each state gives a damage or a benefit. For instance the height of the river Loire at Orléans follows some probability distribution, and to any given height may be (theoretically) associated the cost of the possible flood.

However this representation by a pair of mathematical quantities is a too simple and too ideal scheme of thinking.

- Too ideal because we are almost never in a situation where this model is well filled in. We dont know the tails of the distributions because the data are lacking on rare events. We ignore the correlations in order to estimate the damages and no complete description of what may happen is available.

- But, above all, the model is simplistic because it is hiding the reasons why we are interested to these events by doing as if they could be translated automatically and objectively into costs.

The true goal of risk analysis is not to do computations but to progress with a little more clear-sightedness in social facts and uses. It may be about the threat a child be run over by a car when crossing the street, about the risk that the air of London poisons the inhabitants, or that the bankruptcy of an firm induces other bankruptcies, etc.

The intellectual operation consisting in probabilizing a situation is fundamentally an ousting, an erasing of the meaning. In the particular case of physics, it is the result of the construction process of the knowledge on the laws of matter. But it is widely problematic for all what concerns the human behavior. The risk analysis is necessarily understanding of interpretations.

Computations, even with methodological precautions, have the innate defect to hide ignorance. As we dont know precisely to quantify neither the return risk of a loan, nor the market or liquidity risk, nor the risks due to human mistakes or due to a regulation change, very accurate computations are mixed with rough estimates hopping they will have no sensitive consequences on the result. During the manufacturing of packages of several credits, a standardization of the description occurs (reduced to the notations of the agencies) which moves away from reality. At the limit, a mortgage loan in Iowa or Kansas is thought in the same manner as a credit at New-York on Madison avenue if they are both well assessed.

As the mathematical theory if far in advance before the available data, sophisticated methods are applied to rough hypotheses whose coarseness is forgotten along the computation. The bankruptcies of the banks or firms are represented by Poisson processes whose parameter, the mean flow, is in fact unknown. It is a characteristic of coherent measures of risk to involve the whole tail of the probability distribution. But the tails are *never* known. Hence their use implies necessarily tacking a mathematical model on an ignored reality.

It is *the meaning* of an event that makes the risk. Let us take an example, suppose a particular type of cancer be enumerated within the population. This subset of the suffering people is the concrete event. The ratio to the whole population will be taken for an estimate of the risk. If it would happen that observation and inquiry reveal that a significant part of the concerned persons have taken – say cannabis – twenty years before, then all the cannabis consumers become potential patient. If another common property of the sick persons is pointed out – e.g. they have used mobile phones as soon as these were available – then almost the whole population is now threatened. The risk changes when the interpretation modifies the meaning of the event.

Reducing the risk to a probability distribution and a money amount rubs out the main part of the difficulties. It is to attach confidence to the mathematization as *approximation*, as if the physical reality was concerned, but the question deals with *meaning* whose subjectivity impregnates all the social relations of the agents. We are not in an approximation process, as usual in applied mathematics with discretization or finite elements methods. It is interpretation, hence *meaning* which is transformed into numbers.

We said that significant improvements have recently been brought to these procedures thanks to the coherent risk measures. *But all these methods have the inborn inadequacy of considering that the interpretating process is closed.* Now on the contrary, instead of being closed, it is in permanent innovation. As soon as a new reading appears, it generates new risks which are only perceived by those ones who understand it. If in 2006, nobody sees the increase of the real estate prices and the decrease of the households savings in the US as a phenomenon allowing several readings, the corresponding risk is not detected. The mathematization sweeps up these difficulties thanks to hypotheses on the tails of the distributions. It is not enough to say that they are badly known. They are, by nature, temporary and fluctuating depending on the interpretative knowledge that the agents acquire by their understanding of economical phenomena.

This limitation of the relevant mathematization holds as well for the other risks due to scientific and technical innovations on which focused Ulrich Beck as soon as the 1980's.

In fact any advance in the knowledge makes us discover features of the world on which we were not aware before and, by this, generate new risks. Whatever way is taken for mathematizing the risks, they are congealed, canned in a box which hides the interpretative. It is compulsory, the weakness of mathematics is that they are formal !

III. Can we transpose these arguments to the economical models of global change ?

Uncertainties are so numerous about CO_2 future concentration that IPCC, in his methodological recommendations to contributers, looks for a clear and well calibrated language to speak on vague notions. Is it the right way? As things are taken, it is doubtful[7] .

First of all, we can think that it is possible to avoid any assumption on the tails of the distributions, as with the criterion VaR, by reasonning inside a domain with explicit boundary.

In a modelling framework, the frontiers are defined by specifying a domain to all the quantities: to the data, to the parameters, and, thanks to the model to the computed outputs. It is possible to write down a methodology BaR (Boundary at Risk) which extends VaR, and consists for each quantity, say X, to define a domain D and a threshold

[7] It is suggested 15 steps in order to take in account the desirable precautions from the beginning of the work until its final expression, evaluating the doubts, the ignorances, the uncertainties and the degrees of confidence which may be attached to each part of the discourse. Three categories of uncertainty are distinguished: unpredictability, structural uncertainty, uncertainty on the numerical values. This philosophy adopts the rule of claiming as few as possible and using mood auxiliaries watering down any remarkable insight in order all what is interpretative desappears. For instance the degrees of confidence are splitted in five levels: Quantitatively calibrated levels of confidence

Terminology	Degree of confidence in being correct
Very High confidence	At least 9 about 10 chance of being correct
High confidence	About 8 out of 10 chance
Medium confidence	About 5 out of 10 chance
Low confidence	About 2 out of 10 chance
Very low confidence	Less than 1 out of 10 chance

This rather curious table shows how the care of well doing may leads to obscure nomenclatures: how is it possible to have a low degree of confidence in a model which has 2 out of 10 chances of being correct? With 5 chances on 10 already, the model doesn't yield information any longer, lower it brings a thesis about what will not happen. We see that the two discourses of the expert and of the model are confused. The calibration proposed by the IPCC is a false rigour. It simply reduces to carefully evade any formal criticism.

This point has been objected in the CIRED: "have we not to consider that a model correct one times over ten yields an information? If it rains 1 day over 2, and if some model, correct one times on 10 predicts the rain, we can say that the probability of the rain is greater than 1/2." This objection shows the difficulty to keep clear ideas even among scientists about the meaning of words. For myself I argue in the following way: if the model is correct 1 times on 10, it is wrong 9 times on 10, then if it says today "rain", there are 9 chances on 10 that the sun shines. In the objection the expression "to be correct one times over ten" is taken as meaning "the model has a correlation greater than 1/10 with the reality".

of probability p such that $P\{X \notin D\} \leq p$.

This way of thinking possesses the natural following coherence: if several situations are considered depending on a parameter α and if

$$P_\alpha\{X \notin D_1\} \leq p$$

for α in the domain D_2, if α becomes random and if the probability that α be outside D_2 is less than q, then for the resulting probability one has evidently:

$$P\{\alpha \notin D_2,\ X \notin D_1\} \leq p + q.$$

The language *BaR* allows to improve a model by taking in account new aleas provided that the new randomized parameters be managed in the same way. Therefore this language doesn't have the defects explicited by Weitzman 2007.

More generally, it is easy to see that if two random quantities are considered X and Y and if $N(y, dx)$ is the conditional law of X given $Y = y$. If we know

i) the measure $N(y, dx)1_{D_1}(x)$ when y is in D_2

ii) the law of Y given the event $Y \in D_2$ i.e. the probability measure $\frac{1_{D_2}(y)P_Y(dy)}{P_Y(D_2)}$

then we know the joint law of the pair (X, Y) given the event $\{X \in D_1\}$ and $\{Y \in D_2\}$.

Despite several interesting works related to this way of reasonning (cf. Petschel-Held et al. 1999, Bruckner et al. 2003, Corfee-Morlot et al. 2003), this approach presents necessarily the counterpart of its advantages : nothing is said in case of overcrossing the boundary. In fact, with a suitable mathematical formalism, BaR may be seen as a special case of VaR, so that it may be subject to the same citicism as VaR in particular to give no information on what happens when the boundary is crossed. But the ultimate goal of the models is to favor the emergence of an intersubjective truth in order that the parties, inside their interest conflict, may begin a constructive dialog. The philosophy of *Boundary at Risk* can only be relevant if everyone hopes to remain in the studied domain and acts for this at any cost. But the action of economic forces makes instead that everyone is oriented toward a progress of his energy consumption what leads straitforward to the boundary. In other words communication rules are of the first importance, as is well known in matter of climate change, and this imposes that extreme events occuring behind the boundary be described and discussed.

The logic of scenarios suggested by the "coherent measures of risk" is much more promising. We find here an approach which was already in practice among civil engineers. Since it is actually impossible to explore all the branches of the tree of consequences leading to the ruin of the structure, some *a priori* damagings are considered and the probability of the scenarios leading to such damages is estimated. This logic of scenarios penetrates more and more in every field of the futurology, and especialy in the studies of the relations between economic decisions and climate modifications.

In the case of the damaging of a civil engineering structure, for security reasons the engineer will choose the unfavorable envelope of all the considered scenarios. It is also what bankers do for estimating the exposure of their company.

But in the case of economical models for the global change, it is rather the specific appearence and the qualitative description of the consequences of each scenario which is liable to interest the decision makers.

Of course these approaches have the defect which is the counterpart of their advantages: they use tails of distributions which are badly known and hence may be criticized. In addition, we must keep in mind that *the main defect of the methods using tails of distribution is not so much to propose an imperfect representation of the risks but rather to let believe that the interpretative process is closed.* But continuously the agent think to improve their knowledge and forecastings, and for this re-interpret all the information they capture.

This is well illustrated by the language used by the IPCC. If we read today the first report of 1990, the supplement of 1992 and the second report approved in December 1995, we are necessarily amazed by the fact that the meaning of the tails of distribution changed. They reflected a high ignorance on physical phenomena so that a lot of accelerating effects were unsuspected. They reflect today much more the doubts about the behavior and the political decisions.

Sometimes a distinction is done between the alea related to the (natural and social) reality and that one related to our own ignorance which may be more or less "probabilizable". The point is that generally the statistical methods do not allow to share clearly between these two categories which in practice overlap. But is that really the problem? In any way it is an ignorance about the plausibility of a phenomenon, the main point is that we are interested by this phenomenon because it means something.

If we consider a very elaborate model of the type of IMAGE for instance and that a lighter simulation is proposed which reveals new phenomena like the permafrost fermentation, or the lubrification of the rock by the water under the glaciers, etc., all the preceding formalization of the risk becomes lapsed because new significations appear. A modelization is always an interpretation[8]. Already for natural phenomena it is impossible to be exhaustive and new effects are always possible, but this is yet much more relevant in the case of modelization of economic phenomena. The more physical phenomena of climate change are better known the more we are closer to a similar situation to finance where it is the interpretations and anticipations of the actors that we have to put into the risks. The hope of tending to a "reality" thanks to a finer and finer mathematization is an illusion because in mathematizing the economic risks one attempt truly to modelize all the possible interpretations of a situation.

The investigation modelling has the ambition to reveal new significations.

In some sense, it is with respect to heavy mathematization what sociological enquiry is with respect to statistical poll. Let us explicit this comparison. Econometricians often criticize sociologists that they content themselves of polling about fifty persons, an unsufficient sample of the population to deduce anything. On the contrary they construct polls, as for them, statistically exploiting thousand or more questionnaires. But the sociologist doesn't proceed with questionnaires, he lets people talk. What he is looking

[8]Cf. N. Bouleau *Philosophies des mathématiques et de la modélisation*, L'Harmattan 1999.

for, when he investigates on daily transportation or on the use of water by the inhabitants for instance, it is explanations to which he wouldn't have thought *a priori*, significant reasons which enlighten some behaviors and which he will use later on to deepen the subject. The questionnaire encapsulates thought categories. That yields numbers, ratios, but it is closed. It is not with its help that what is not yet imagined will be discovered.

Therefore investigation modelling possesses, from this point of view, a fundamental justification. It is not a clumsy sketch of truly scientific models which run gigantic black boxes. It has an essential epistemological role, that of regenerating the risks signification. For that it can avoid the heavy, accurate and opaque quantitative which has the innate flaw of a hight intellectual inertia, it can remain light, movable following the aim of making significant phenomena appear.

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